STATISTICAL TECHNIQUES FOR QUALITY IMPROVEMENT

BY

SRINIVAS B. GARUDACHAR

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APPROVED BY:

(MAJOR PROFESSOR)

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I. INTRODUCTION

Due to intense market competition, there is a need to produce better and chaaper products. This can be accomplished by applying Statistical Quality Improvement techniques, simple and sophisticated, in an effort to learn more about the product and the process. Simple tools, widely applied, are essential for refining and improving products that are baing manufactured. These tools can only partially compensate for the flaws caused by faulty design that might have occured at the early stages of the product or process development. In order to maka improvements in quality , one needs to experiment with alternatives in the early development stages of the product or process in order to achieve a sound basic design, so that fundamantal flaws or imperfections do not occur later. This kind of experimentation can be expensive, but the expense can be justified if it results in a product of better quality.

In designing quality products, a high degree of efficiency can be attained through the application of Statistical Design of Experiments(SDE). In the context of the high utility value of SDE in product and process quality improvement, the ideas of Taguchi are noteworthy of mention.

The concept of quality and its improvement should be a top priority of every person in a company. This requires good teamwork and the use of certain scientific methods. Producing quality goods and increasing the productivity at minimal expense, can be accomplished by having as complete a knowledge as possible about the process and by being able

to use such knowledge to advantage. In other words, Quality Control is not just a passive inspection of products. One has to relate the results of the inspections to whatever knowledge one might have about the process and gain further insight into the possible causes of the defect, and devise means of circumventing the problem.

In order to discover something of importance, one needs to be well informed about the subject and at the same time be a keen or attentive observer. According to Box[1987], we need a critical event, one that contains significant information; but obviously one cannot observe the event if one is not a keen observer. In general, most things that occur are not critical and not much is learned from them. Every now and then, however, some exceptional events do occur. Perceptive observers are rare, too. The requirements for being a perceptive observer are that one should have an innate natural fascination or curiosity and at the same time, be well versed in his area of work.

In order to make a revolutionary technological change, one has to increase the probability of the occurence of a critical event and a perceptive observer coming together. One way to accomplish this is to make sure that there is a free flow of naturally occuring information to the observer. Another way is to have directed experimentation, which could increase the chances of the occurence of an informative event so that it is more likely to be observed.

Some of the tools or ways of focussing ones interest on the quality of the product are check sheets, Pareto charts, histograms, scatter plots and graphs. All of these tools are rather simple and eye catching, and can be understood by everyone.

As an example, suppose we find that in a week of production of springs, about 75 are defective. This observation by itself might not be helpful in improving the process. On the other hand, if one inspects the defective springs, categorizes them by the nature of the defects: eg. scratches, cracks, pinholes, dimensional inaccuracies etc., and marks them on a check sheet, the results can be shown on a Pareto chart. For instance, a chart might show that the majority of the springs were discarded due to cracks in which case, a solution can be sought to remedy the problem. The Pareto chart serves to separate the few important defects from the numerous trivial ones.

The next step is to look for possible causes of the cracks. This can be accomplished through the use of a cause-effect diagram. This diagram facilitates a focused and educated discussion and an interchange of ideas among observers about what might be the possible causes for the cracks. Often this can lead to the identification of a number of causes that need to be analysed. We might categorize the cracks by their size and construct a histogram to show the distribution of cracks of different sizes. This, in itself, can be the summary of the crack problem that we are faced with. However, if there are two types of springs, A and B, the original histogram can be split up into two parts, each part showing the frequency distribution for the crack size. From the figure, one can see that most of the cracks occur in(say) type A and also, the size of the cracks are larger in type A. Hence, by this type of data stratification, one can approach the root of the problem and its causes. Scatter diagrams are also

helpful in that they might show the relationship between the crack size and say, the temperature of hardening.

Evidently, being an informed observer is not sufficient in the quest to improve quality. Directed experiments are needed in order to ensure well designed processes and thus well designed products, resulting in greater improvements in quality.

The emphasis of directed experimentation is on obtaining a product and a process that are well designed, so that minimal rejection takes place, thus reducing the need for inspections and paving the way for economic gains.

STATISTICAL DESIGN OF EXPERIMENTS:

Experiment design was introduced in the early part of the 20th century. R.A.Fisher [1966] argued that , for any prototype to be successful in the real world, it ought to be designed and tested in the most adverse of real conditions and not in orchestrated perfect conditions simulated in a laboratory. This idea was adopted in a multitude of fields such as Medicine, Biology, Education and to a lesser extent, Engineering. Unfortunately, the potential of reaping the benefits of the experiment design has not been properly exploited by the industry in the West; mostly due to the lack of understanding and the support on the part of the management. The basic difference between the policies of the managements in this country and those in Japan, lies in the fact that the Japanese management mandates the

requirement that statistical design methods be used in order to develop a quality product.

The Japanese have been successful in building quality into their products and processes. In particular, Taguchi [1978] has emphasised the importance of using Statistical designs in :

i) Minimizing variation with mean on the target.

ii) Making products robust to environmental factors.

iii) Making products insensitive to component variation.

iv) Life testing.

The first three categories are examples of what Taguchi terms Parameter Design. He also advocates the use of signal-to-noise ratios (S/N), accumulation analysis, and minute analysis. Though some of these methods are extremely difficult in certain cases, they may prove to be equally inefficient and complicated in some other cases. So, in order to decide whether or not to make use of his methods, one needs to be judicious and somewhat selective.

i) MINIMIZING DEVIATION FROM THE TARGET:

In his analysis, Taguchi recommends that the S/N ratio be used as a measure of the product performance. This is given as

SNT = 20 $\log(\bar{y}/s)$; where, $\bar{y}$ is the mean of the individual observations and s is the standard deviation.

According to Box[1987], this S/N approach seems very complicated and restrictive. Its use is equivalent to using the logarithmic

transformation which renders the standard deviation independent of the mean. It must be noted, however, that the use of the log-transform is not always justified. Sometimes, the analysis can be made by using no such transformation or by using some other type of transformation like a reciprocal or a square root. Hence, unless the log-transform is the right transform, the S/N ratio will be inappropriate.

ii) ROBUSTNESS TO ENVIRONMENTAL CONDITIONS:

Taguchi stresses experiment design to make products robust to environmental factors. Designing experiments to make the products robust to environmental variations involves a combination of designs called inner and outer arrays. For each experimental run in the design (or inner) array where the primary design factors are varied over a wide range, another(outer) array of runs is conducted by varying other environmental conditions or factors to create noise. Such designs require a large number of experimental runs but the results can be very informative.

iii) PRODUCTS INSENSITIVE TO COMPONENT VARIATION:

Most products are assembled from a number of components. The performance of the final product will vary if the individual components vary. One needs to modify the design of the finished product in a way that minimizes the effects of component variation. Taguchi illustrated his approach of parameter design with a Wheatstone

Bridge circuit.(see chapter 6) The goal was to choose the nominal values of the various resistances, the battery voltage, etc. such that the unknown resistance y, could be determined as precisely as possible. In this example, the mathematical relationship between the characteristics of the components and of the response is known from physical theory, so, computer calculations can take the place of experiments. However, Taguchi uses inner and outer array techniques together with S/N ratios to solve the problem. Box [1987] pointed out that this method does not necessarily yield the optimal solution and that S/N ratios are not needed. Instead, he suggested the use of some standard optimizing procedures.(see chapter 6 for example.)

iv) LIFE TESTING:

Life testing deals with the product reliability, which is very important to the customer over the lifetime of the product. It deals also with the determination of the probability of failure, which is an integral part of Quality Engineering. Designed experiments to improve product reliability are an important part of Taguchi's philosophy.

Box points out that though Taguchi's philosophy and concepts have been invaluable in encouraging the use of experiment design in improving quality in Engineering, the methods of Taguchi are often cumbersome and/or inefficient and one has to be careful in deploying them. Often, there are simpler and more efficient methods that could be employed.

Modern quality improvement requires an effective collection and use of observational data and an effective use of design of experiments. Numerous experiments have to be made to develop a product design that seldom goes wrong. This requires trying many factor-combinations at the design stage, and can be expensive. However, this can be justified in the long run through improved product quality, reliability and longevity.

In conclusion, quality in industry can be improved by teaching Engineers how to apply the statistical methods and designs, to product and process design.

II. OFF-LINE QUALITY CONTROL: PARAMETER DESIGN AND THE TAGUCHI METHOD.

In order for any manufactured product to be a success in the competitive markets, there must be an element of innovation and originality required in the product design. It is well known that it is very expensive to attempt to control the noise or the uncontrollable factors in a manufacturing process. On the other hand, it is less expensive in the long run to optimize the functional characteristic of the product such that it has minimal sensitivity to noise.

A Product's performance varies during its lifespan. Off-line Quality Control(OQC) strives to reduce these fluctuations in order to achieve steady performance and to increase the longevity of the product. Factors that critically affect the product's performance or monetary value have to be identified and separated from those that are less likely to contribute to the improvement of the product performance. These factors or parameters, when used in certain combinations, are found to minimize the loss.

The American Society for Quality Control defines Quality as " the totality of features and characteristics of a product or service that bear on its ability to satisfy a users given needs". Performance characteristics are the final characteristics of a product that determine the product's performance in satisfying a user's needs. Loss occurs when a product's characteristics deviate from a desired or idealized target value. Along with specifying the targeted value, one

must also specify the tolerance level allowable for that characteristic. The whole idea is to minimize losses, i.e. to have minimal changes in the performance characteristics about the target during its life span under different operating conditions.

The main causes of a product's performance variation are: 1. Environmental factors, 2. the deterioration, decay or depreciation of a product and 3. the manufacturing defects. Changes in temperature, humidity, etc., are examples of environmental factors. Examples of product deterioration are wear and tear of moving parts or loss of stiffness in a spring. Manufacturing defects are present due to the obvious differences in machines that manufacture the same item or it may be due to machine operators differing skill levels. The design of the manufacturing process and the product are important factors that determine the degree of performance variation and also the manufacturing cost.

The three different but yet overlapping stages in design are: a) Product design, b) Process design and c) Manufacturing design. Traditional Quality Control Methods concentrate largely on the manufacturing process by trying to reduce manufacturing imperfections in the product. This is called On-line Quality Control. Off-line Quality Control is used at the process and product design stages in order to improve manufacturability and reliability and to reduce costs. Specifying the tolerance alone(as in on-line Q.C) could be misleading. It is necessary that a specific target be specified, in addition to the tolerance levels. It is imperative that all the product's characteristics are at their ideal(target) values for a good

performance. A product performs best when all the characteristics of the product are at their ideal values. Knowledge of the ideal values of the product and the deviation from these values gives us an insight into the losses and serves as an incentive to improve the quality.

According to Taguchi (1978), Quality Engineering Design basically consists of three steps :

a) System Design

b) Parameter Design

c) Tolerance Design.

Parameter design is most crucial for achieving an optimal balance between high quality and cost. The goal of Parameter Design is to determine a combination of controllable factors that result in accomplishing the task best, with minimal influence due to changes in uncontrollable (noise) factors.

The quality of a product is very dependent on the external factors contributed by the environment or even on psychological factors like consumer dissatisfaction, the firm's reputation and so on. The quality of a product can then be ascertained only after a full assessment of all developments that can occur, from the time the product is shipped out, has been obtained.

Loss Function:

Loss occurs when a product deviates from its target value. This loss, being a function of its deviation from the target, is assumed to be quadratic [Taguchi, 1978], namely

$$l(y)=k\ (y-\tau)^2\ ; \qquad (1)$$

where l(y) is the loss function, k is the cost coefficient, and y is the quality characteristic which is a random variable with a certain probability distribution.

The expected loss is defined as

$$L=E(l(y))=k\ E[(y-\tau)^2]. \tag{2}$$

where the expectation is taken with respect to the distribution of y, during its lifespan.

For cases when l(y) is not symmetric about τ,

$$l(y)=kl\ (y-\tau)^2 \quad \text{if } y<\tau; \tag{3}$$

$$=k2\ (y-\tau)^2 \quad \text{if } y>\tau. \tag{4}$$

$$L = E[l(y)]= k_1P_1E[(y-\tau)^2/y<\tau] + k_2P_2E[(y-\tau)^2/y>\tau] \tag{5}$$

where, $P_1= P\ [y< \tau]$ and $P_2= P\ [y> \tau]= 1-P_1$.

Parameter design consists of discovering the combinations of product design characteristics that minimize loss. This may be done experimentally or by simulation. The first step is to identify the design parameters and the noise sources. The former are product design characteristics, whose combination is specified by the product designer (with tolerances). A vector of combinations of the design parameters defines a product design specification and vice versa. The design parameters in a manufactured product may have some tolerances and may deviate from the nominal settings. Noise factors are nuisance factors that are difficult or impractical to attempt to control; they are primarily environmental factors or are due to manufacturing imperfections or product deterioration.

Uncontrollable or noise factors can be categorized as outer noise, inner noise or between-product noises.

An outer noise can arise due to consumer, usage conditions, fluctuations in temperature and / or vibrations, humidity, or natural differences in skills of operators.

An inner noise may be due to depreciation and deterioration, aging and wear of tools and materials.

A between-product noise arises from piece to piece variation when the product is not homogeneous.

Parameter design is possible because the effects of both the external noise and the internal noise can change with the combinations of design parameters. Thus,it is possible to identify a combination of design parameters, that reduce performance variation. A Parametric design experiment consists of two parts: a) a Design matrix and b) a Noise matrix.[Kackar,1985]

The columns of a design matrix represent the design parameters and the individual entries in the columns represent test setting values of the design parameters with each row representing a product design.The columns of a noise matrix represent noise factors and the rows represent the different combinations of the levels of these noise factors. See fig. 1 for an example of a parameter design experiment.

A parameter design experiment consists of a combination of design and noise matrices and if they have m and n rows respectively, the total number of rows in the combined experiment is m x n.

For each row in the design matrix, the n rows of the noise matrix provide n repeated observations of the performance characteristic. The

levels of noise factors and the noise matrix are chosen such that the repeated observations are representative of the effects of all possible noise factor levels. The repeat observations in the performance characteristic from each test run in the design matrix, are then used to find a performance statistic which estimates the noise effects. The m values of the performance statistics associated with the m test runs in the design matrix are used to predict the combinations of the design parameters that minimize loss.

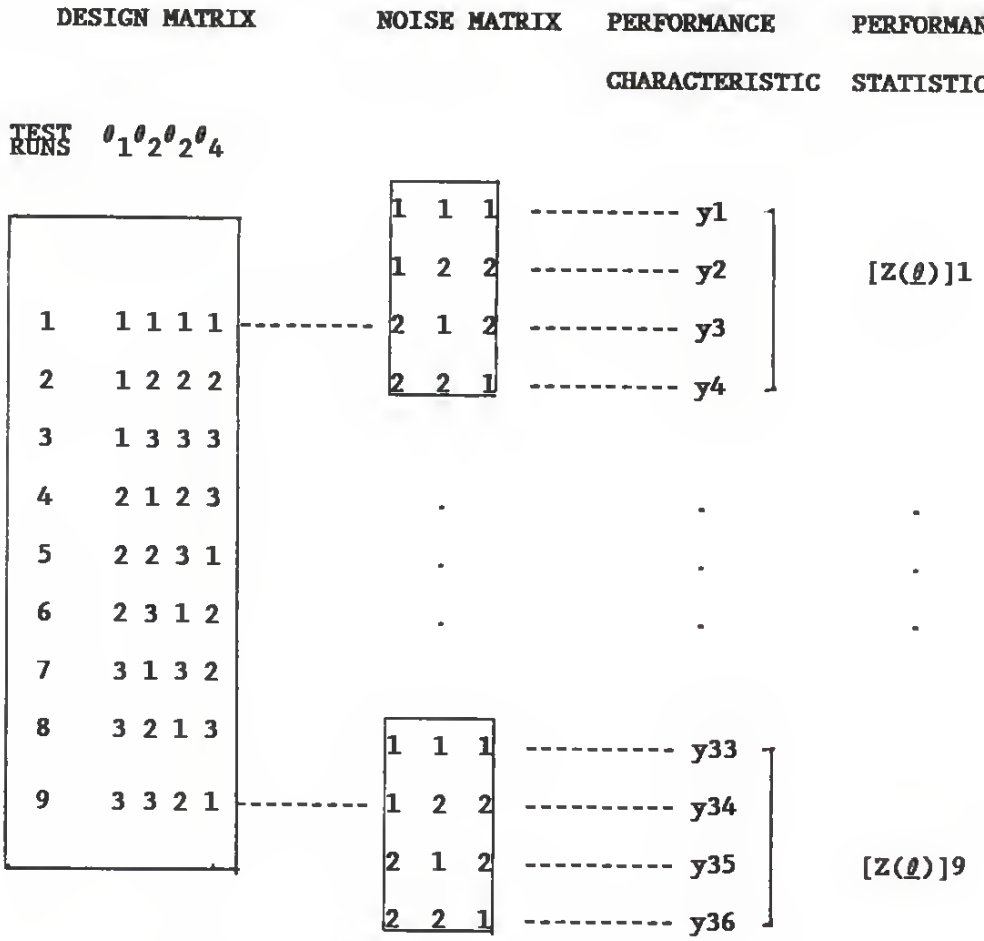

Figure 1. An example of a Parameter Design Experiment.

Let $\underline{\theta} = [\theta_1, \theta_2, \ldots, \theta_k]$, be the design parameters and let $\underline{w} = [w_1, w_2, \ldots, w_l]$ be the noise factors in the parameter design experiment. Assuming that the performance characteristic, y, is a function of $\underline{\theta}$ and $\underline{w}$, then $y = f[\underline{\theta}, \underline{w}]$. For a given $\underline{\theta}$, the noise factors generate the distribution of y. Let,

$$N(\underline{\theta}) = E(y) \text{ and } \sigma^2(\underline{\theta}) = E[(y-N(\underline{\theta}))^2] = \text{var}(y). \tag{6}$$

Thus the expected loss is a function of $\underline{\theta}$. A performance measure is that function of $\underline{\theta}$ which when maximized, results in a minimal expected loss. An efficient performance measure takes into account all positive Engineering information about the product.

A performance measure is an unknown function of $\underline{\theta}$ and has to be estimated prior to optimization. Taguchi[1978] uses signal to noise ratios as statistical estimates of performance measures.

Let

$MSE(\underline{\theta}) = E_y[(y-\tau)^2]$ = mean square error or loss

$B(\underline{\theta}) = N(\underline{\theta}) - \tau$ = bias from target, τ

$\xi(\underline{\theta}) = [N(\underline{\theta})]^2/\sigma^2(\underline{\theta})$ = square of the inverse of the coefficient of variation of the distribution of y.

The loss function takes on one of three forms, depending on whether a smaller target is better, a larger target is better, or if a specific target value is best.

1. The smaller the target, the better the product: In this case, $\tau=0$ and $MSE(\underline{\theta}) = E(y^2)$.

The Taguchi performance measure is taken to be

$\Phi(\underline{\theta}) = -10 \log(MSE(\underline{\theta}))$;

the performance statistic is $Z(\underline{\theta}) = SNS = -10 \log[\Sigma\, y_i^2/n]$.

2. The larger the target, the better the product: In this case, $\tau \to$ infinity and $MSE(\underline{\theta}) = E(1/y^2)$

The Taguchi performance measure is $\Phi(\theta) = -10 \log(MSE(\theta))$ and the performance statistic is $Z(\underline{\theta}) = SNL = -10 \log[\Sigma(1/y_i^2)]$.

Maximizing SNL is equivalent to minimizing the loss.

3. Specific target value is best: Here, $\tau = \tau_0$ and the performance measure is $MSE(\underline{\theta}) = E[(y-\tau_o)^2] = \sigma^2(\underline{\theta}) + [N(\underline{\theta})-\tau_o]^2$.

In many applications, the mean and the variance of y ($N(\theta)$ and σ^2respectively) are independent of each other. The bias, $B = N(\theta) - \tau$, can then be minimized independent of the variance by adjusting special design parameters that have an effect on $N(\theta)$ but not on $\sigma^2(\theta)$.

When the mean and the variance are independent, an efficient performance measure could be $\Phi(\theta) = -\log[\sigma^2(\theta)]$; the negative sign indicates that maximizing $SNT = -10 \log(s^2)$, would minimize s. In some other cases, the mean $N(\theta)$ may increase linearly with $\sigma(\theta)$. Taguchi [1978] has found that, in this case, the bias can be reduced

independent of $\xi(\theta)=(\sigma(\theta)/N(\theta))$, the coefficient of variation, by adjusting some of the design parameters that have a marked effect on $N(\theta)$, but not on the ratio (i.e. the coefficient of variation).For this case Taguchi used a performance measure, $\Phi(\theta) = -10 \log[\xi(\theta)]$. For this measure, the smaller the coefficient of variation, the larger will be the performance measure. Taguchi recommended two performance statistics

(S/N ratios):

$$Z1(\theta) = SNT_1 = 10 \log(\bar{y}^2/s^2)$$

and

$$Z2(\theta) = SNT_2 = 10 \log[(\bar{y}^2/s^2)-1/n]$$

These two statistics are equivalent in that , maximizing either one would minimize s. Bias from target can then be minimized,by adjusting the appropriate design parameters or control factors.

The following are a few guidelines in order to select the adjustment parameters to maximize the performance statistics. [Kackar, 1985].

1. All design parameters and their combinations must be identified and the noises and their ranges must be known.
2. The design and the noise matrices must be set up as a plan for the parameter design experiment.
3. Evaluate the performance statistic for each combination of design parameters.
4. The values of the performance statistics must then be used to predict new and better combinations of design parameters.

The above steps are discussed in more detail.

1. Initial settings of design parameters are obtained from the system design($\underline{\theta} = \theta 1, \theta 2, \theta 3, \ldots, \theta k$). The design has to identify other possible $\underline{\theta}$ values in order to form the parameter space $\underline{\beta}$. The noise factors, that cause a significant amount of performance variation, have to be identified and the ranges of the noise factors, within which product performance is desired to be insensitive, have to be evaluated. A set of such noise factor combinations constitute the noise space. For a given $\underline{\theta}$ in the parameter space $\underline{\beta}$, different levels of noises are used in the experiment to obtain the values of the performance characteristic. These values show the effect of noise on the performance characteristic and are useful to compute the performance statistics. Typically, a value of $\underline{\theta}$ that maximizes the performance statistic (S/N ratio) with no resulting cost differential, is chosen.

2. A full search of the parameter space is not practical. For this, one needs to be judicious as to the choices made of the values of $\underline{\theta}$, in order to build the design matrix. The columns represent the different parameters chosen and the rows represent the various parameter combinations. For each row, a performance statistic has to be calculated. The design matrix should be selected such that the calculated values for the performance statistics provide good information about a particular $\underline{\theta}$ in the parameter space in a minimum number of tests.

Generalized Graeco-Latin squares(orthogonal arrays) can be used to construct the design matrices. Three or more settings must be chosen

for each design parameter in order to reveal non-linearities in the design parameter main effects. Orthogonal arrays can also be used for the noise matrix, constructed by a judicious and a representative sampling of the noise space.

3. A simulation of the parameter design experiment can be evaluated, when $y= f(\underline{\theta},\underline{w})$ is known. Product performance can be evaluated in the presence of both internal and external noises. The function, $y=f(\underline{\theta},\underline{w})$ is evaluated for each $\underline{\theta}$ in the design matrix and each $\underline{w}$ in the noise matrix. The values of y are then used to find the performance statistic for each $\underline{\theta}$ in the design matrix.

4. The computed values of the performance statistics are used to predict $\underline{\theta}$ in the parameter space that yields a maximum performance statistic. Though the noise matrix is a selective set, the repeated observations on y, for the given $\underline{\theta}$, obtained from a noise matrix approximate a random sample from the distribution of y given $\underline{\theta}$.

III. S/N RATIOS, PERFORMANCE CRITERIA AND TRANSFORMATIONS.

Taguchi's analysis of designed experiments utilizes the S/N ratios for performance criteria. The aim of this analysis is to satisfy the following criteria:

i> The quality characteristic of the product must be as close to the target value as possible.

ii> The products sensitivity to variations in components must be minimal.

iii> The products sensitivity to variation caused by environmental quirks must be minimal.

These aims are very important in the improvement of quality, [Box(1987)]. But the Statistical analysis and design as performed by Taguchi are often too inefficient and/or too complicated requiring them to be replaced by some simpler and more efficient methods.

Here we consider the three performance criteria:

1. The response is as close to the target value as possible.

$$SNT = 10 \log(\bar{y}^2/s^2) \tag{1}$$

2. The objective is to make the response as large as possible.

$$SNL = -10 \log[\Sigma_i(1/y_i^2)] \tag{2}$$

3. The objective is to minimize the response.

$$SNS = -10 \log[1/n(\Sigma_i\, y_i^2)] \tag{3}$$

In terms of the population parameters,

$$SNT = 10 \log(\mu^2/\sigma^2) = -10 \log(\gamma^2)$$; where γ is the coefficient of variation = σ/μ. (4)

Phadke[1983] states that the motivation to use the S/N ratios instead of simply using the standard deviation as a performance criterion, is that the mean and the standard deviation frequently vary in direct proportion. Thus, if the standard deviation alone were used, optimization would be difficult - this is so since it involves minimizing the standard deviation first and then bringing the mean close to the target which causes a problem, when the mean and the variance are correlated. The Taguchi-two-stage optimization involving the S/N ratios yields the level of parameter combinations which have a small standard deviation and a mean in close proximity to the targeted value. This is based on the observation by Taguchi(1978), that when mean and variance are related, the mean and the S/N ratios are not!

The Taguchi approach is based on the premise that there exist:

a. Control factors, that effect the process variability, i.e. the S/N ratios.

b. Signal factors that have little or no effect on the variability but have a substantial effect on the mean.

c. Other factors that do not affect S/N or the process mean.

Leon Low[1987] states that if the factors can be classified as above, then the S/N would be a performance measure independent of adjustment, which he termed PERMIA. Here, the signal factors are the adjustment factors, and the control and signal factors together are classified as design factors.

Based on the above considerations, one may let σ and μ be related in such a way that a function of $\mu(\underline{x})$ can be found for which $\{(\sigma^2(\underline{x}))/[f(\mu(\underline{x}))]^2\}$ is a measure of dispersion $P(\underline{x}1)$ - a function of $\underline{x}1$, which in turn is a subset of the design variables, $\underline{x} = [\underline{x}1, \underline{x}2]$. [Box, 1987] Then, $P(\underline{x}1)$ is independent of μ because for a given $\underline{x}1$, μ is a function of $\underline{x}2$ alone. But, P is not a function of $\underline{x}2$, so

$$P(\underline{x}1/\text{given } \mu) = P(\underline{x}1) \tag{5}$$

Here, $P(\underline{x}1)$ is a performance measure independent of adjustment (PERMIA) and $\underline{x}2$ are the adjustment factors that can be altered without affecting the dispersion.

Let $M(\underline{x})$ be the response function that has to be minimized. From the above considerations, $M(\underline{x})$ may be expressed as

$$M(\underline{x}) = [f(\mu(\underline{x}))]^2 P(\underline{x}1) + [\mu(\underline{x}) - T]^2 \tag{6}$$

For fixed μ, $M(\underline{x})$ has to be minimized with respect to $\underline{x}1$, when $P(\underline{x}1)$ is minimized.

If $P(\underline{x}1)$ has a unique minimum at some $\underline{x}1 = \underline{x}10$, the absolute minimum of $M(\underline{x})$, may then be found by changing $\underline{x}2$ alone. The important assumption here is that there is a point on the plane- $\underline{x}1 = \underline{x}10$, which minimizes $M(\underline{x})$ with respect to μ within the region of interest. Thus, $M(\underline{x}) = E[y(\underline{x}) - T]^2$ may be minimized in two stages,

first by adjusting $\underline{x}1$ to minimize $P(\underline{x}1)$ or some monotonic function of $P(\underline{x}1)$ and then by adjusting $\underline{x}2$ such that

$$\mu = \mu 0 - T - f(\mu o)\ f'(\mu o).P \qquad (7)$$

The second quantity in equation 7 is the deviation of μo from the target T and is usually small.

The coefficient of variation $\gamma = \sigma/\mu$ is approximately proportional to the standard deviation of ln y. In particular, if Y= ln y has a mean μ_Y and a constant variance σ_Y^2;

$$\text{then } \mu = \exp[\mu_Y + 0.5\ \sigma_Y^2] \qquad (8)$$

$$\sigma = \mu\ \exp[(\sigma_Y^2 - 1)^{0.5}] \qquad (9)$$

where μ and σ^2 are the mean and the standard deviation of y. From the second equation it can be seen that the coefficient of variation (σ/μ) is independent of μ and the analysis of (σ/μ) is equivalent to the analysis of $\sigma_Y = \sigma_{\ln y}$. The same arguments can be applied equally well for logarithms to base 10 instead of ln. If s_Y, the sample estimate is used in place of σ_Y, then $s_Y = s_{\ln y}$ has a standard deviation which is proportional to its mean. Thus on the hypothesis that a log transform will stabilize the variance , this leads to the analysis of $\ln s_{\ln y}$, which is almost equivalent to SNT, since

$$SNT = -10 \log(s^2 / \bar{y}^2) = \text{constant} - \text{constant}.\ln s_{\ln y}.$$

In the foregoing case, Y= ln y, from which SNT is independent of μ. Let Y= h(y) be a transformation that stabilizes the variance, such

that $\sigma_Y = h'(\mu).\sigma_y$ is independent of μ and also suppose that σ_Y^2 is a function of only $\underline{x}1$, a subset of design factors $(\underline{x}1, \underline{x}2)$. This leads to the problem of minimizing

$$M(\underline{x}) = [h'(\mu(\underline{x})]^2 \sigma_Y^2(\underline{x}1) + (\mu(\underline{x}) - T)^2 \qquad (10)$$

This can be done in two steps by first adjusting $\underline{x}1$ to minimize the variance σ_{Yo}^2 and then adjusting $\underline{x}2$ so that μ takes the minimum value, $(\mu o) = T + (h'(\mu o))^{-3} h''(\mu o)\ \sigma_{Yo}^2$ (11)

In practice, it is more convenient to conduct the analysis in terms of the transformation, $Y = h(y)$. To allow approximately for the fact that $h^{-1}(\mu_Y)$ is a biased estimate of μ, the second order Taylor expansion is employed.

$$h^{-1}(\mu_{Yo}) = \mu o + 0.5\ (h'(\mu o))^{-3} h''(\mu o) \sigma_{Yo}^2 \qquad (12)$$

In the last 2 equations, the second order terms may be adequately approximated by taking derivatives at target(T) rather than at μo. After combining equations (10) and (11), one finds that the manimum mean square error in the original observations, y, will be obtained by adjusting $\underline{x}1$ to minimize σ_Y and then using $\underline{x}2$ to adjust μ_Y to μ_{Yo},

where $$\mu_{Yo} = h(T + 1.5(h'(T)^{-3})\ h''(T).\sigma_Y^2) \qquad (13)$$

If $Y = y^{\lambda}; (\lambda \neq 0)$; then $\mu_{Yo} = \{T[1 - 1.5(1-\lambda)\lambda^{-2}\sigma_{Yo}^2 T^{-2\lambda}]\}^{\lambda}$ (14)

and if $Y = \ln y$, then $\mu_{Yo} = \ln T - 1.5\ \sigma_{Yo}^2$ (15)

While considering the usefulness of SNT, SNL and SNS - it is important to distinguish between two issues 1. the choice of a good performance criterion and 2. the best way to estimate it. A performance measure that is related to the objective must be found and then it has to be optimized to achieve the desired results. Once the performance measure is arrived at, it is necessary that it is estimable with great efficiency.

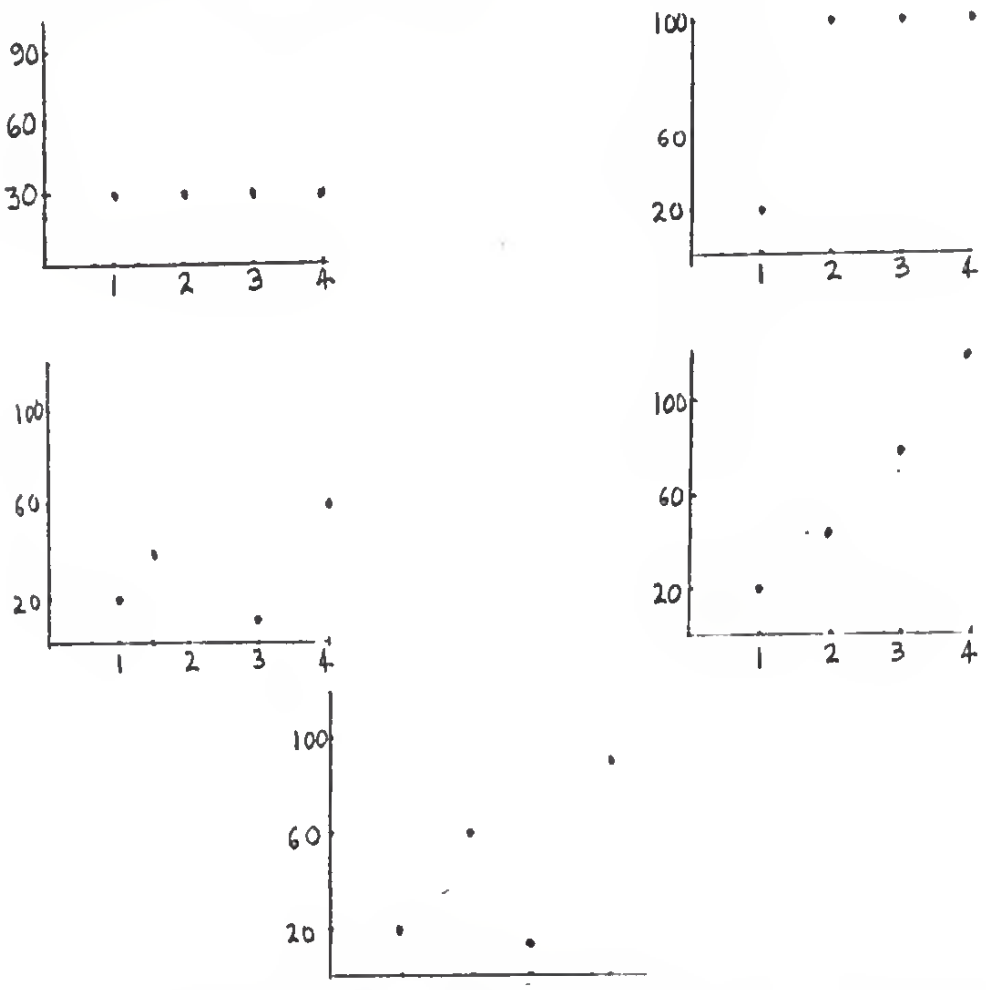

Figure 1. Five samples of 4 observations each, giving the same value of SNL.

The SNL = -10 $\log[\sum_i(1/y_i^2)]$ criterion is used for experiments where a larger response is better. Figure 1 shows 5 samples of 4 observations giving the same value of SNL.

A similar set of 5 samples can be obtained having the same SNS value but with different locations and dispersions. Both the 'larger the better' and 'the smaller the better' cases are concentrated on the idea of location. The efficiency of SNL as a measure of locational shift may be obtained by considering how much larger the sample should be to yield the same power as $\bar{y}$ (using power curves for t-tests). Box(1987) showed in an example based on the normal distribution, that efficiency(η) of SNL was about 42% for σ=10 and about 30% for σ= 13.3. This was equivalent to discarding 58% and 70% respectively, of the data. The 'smaller the better' criterion (SNS) is motivated by an assumption of a quadratic loss. The efficiency of SNS at σ =10 and σ = 13.3 is about 68% implying that 32% of the data is discarded. The possibility of occasional faulty observations or of an error distribution with heavier tails than normal would justify the replacement of $\bar{y}$ by a more robust alternative.

As a justification of Taguchi's ANOVA tests and other tests that he performed, it can be argued that for selecting an optimum combination of factors, it is not of importance as to whether or not a significant or insignificant factor is included in the combination. However, this raises the issue concerning the efficiency of Taguchi's tests since the optimum combination of levels of factors might contain some superfluous factors whose levels might not have any effect on the

performance. This can be eliminated only by very judicious choices by the designer using all the information available and can be quite arbitrary and inconsistent, varying from designer to designer.

Taguchi's key points in the Improvement of Quality are thus:

1> to achieve minimal variation of a desired quality.

2> the need to design products insensitive to environmental disturbances.

3> the need to design products insensitive to transmitted variation.

In arriving at these goals, one may use more efficient statistical techniques than those proposed by Taguchi.

IV. FACTORIAL DESIGNS, FRACTIONAL FACTORIAL DESIGNS, FIRST ORDER AND SECOND ORDER DESIGNS, AND ORTHOGONAL ARRAYS

In the context of product design, Taguchi has placed considerable emphasis on the use of 'orthogonal array' experimental designs (Taguchi and Wu, 1980; Phadke and Kackar, 1983). A great variety of these designs exist, the most commonly used being the 2^k factorials and 2^{k-p} fractional factorials, the 3^k factorials, and designs constructed from the Latin square, Graeco-Latin square, and hyper-Graeco-Latin square designs. Combinations of these are often constructed to permit factors with differing number of levels to appear in the same experimental program. The balanced incomplete block designs may also be considered orthogonal arrays.

The key to understand the usefulness of one experimental design over another rests in the linkages between the designs and the models chosen to describe the response under study. Even when the objective of the experimenter is not a fitted model, the role of bias in the essential statistics must still be considered. For example, a difference betwen two averages may not fully represent the main effect of some factor if that factor has been studied simultaneously in combination with other factors. Interactions can confound main effects. In practice, any orthogonal array is likely to be superior to some arbitrary collection of experiments.[Stuart Hunter, 1985]

When each of the k factors comprising **x** is continuous (where each x_i is represented by two or more levels of temperature, rpm, concentration, etc.), and when the model ,$\eta = f(\mathbf{x})$ is unknown, the most commonly employed empirical model is either the first or second order Taylor approximation:

First order: $$\eta = \beta_0 + \sum_{i=1}^{k} \beta_i x_i \qquad (1)$$

Second order: $$\eta = \beta_0 + \sum_{i=1}^{k} \beta_i x_i + \sum_{i=1}^{k} \beta_{ii} x_i^2 + \sum_{i \neq j} \beta_{ij} x_i x_j \qquad (2)$$

with k+1 and (k+1)(k+2)/2 coefficients, respectively. The coefficient β_0 is a constant term, the β_i are the first order coefficients , the β_{ii} are the k quadratic coefficients, and the β_{ij} are the k(k+1)/2 cross product(interaction) coefficients. Each model requires, in turn, a first or second order experimental design.

Accompanying each model and experimental design is an analysis of variance(ANOVA) table. The essential purpose of the ANOVA table is to separate the overall variability of the observations (represented by the corrected sum of squares and its n-1 degrees of freedom) into components assignable to changes in the responses caused by changes in the controlled factors **x**, and into variability assignable to variance(i.e. due to random influences). Given that an experimental design has been run, the ANOVA table can often be used to construct an approximating model of the response by selecting parameters found to be statistically significant. What can be estimated, and not estimated, depends on the experimental design. Model parameters that

cannot be estimated can seriously influence(bias) both the model's estimated parameters and the estimate of variance.

FIRST ORDER DESIGNS:

Let η be an unknown function of k factors **x**. If an experimenter is highly confident that , over his experimental region, this function can be represented by a first order model, he need only estimate the k+1 coefficients in the model given by equation 1.

Let $\mathbf{x}_u$ be the u-th experimental point in a k-factor experimental design **D**. Then any collection of N points, with the property that for every pair of factors x_i and x_j the sum of the crossproducts $\sum_{u=1}^{N} x_{iu}x_{ju}$ is zero provides a first order orthogonal design. Further, if the settings of each factor can be arranged so that $\sum_{u=1}^{N} x_{iu}^2 = N$, the estimates of all the coefficients in the first order model will have minimum variance. Additionally, the variance of the forecast $\hat{y}$ will everywhere be a constant at a fixed distance from the center of the design. The design is then rotatable as well as orthogonal (Box and Hunter(1957)). Table 1 gives three equivalent first order orthogonal rotatable designs for k = 2 and N = 4.

TABLE 1. First Order Rotatable, Orthogonal Designs

2-Level		3-Level		4-Level	
x_1	x_2	x_1	x_2	x_1	x_2
-1	-1	-1.414	0	1.13	0.85
1	-1	1.414	0	-0.85	1.13
-1	1	0.0	-1.414	-1.13	-0.85
1	1	0.0	1.414	0.85	-1.13

Orthogonal Arrays

Orthogonality, although an important criterion when fitting first order models, loses its importance when an experimenter must contemplate models with second order(curvature and interaction) components. If the objective of the experimenter is to forecast a response at either the present or future settings of **x**, then an unbiased minimum variance estimate of $\hat{y}$ is required. It was demonstrated that rotatability (Box and Hunter- 1957) and the minimization of bias from higher order terms (Box and Draper- 1959) were essential criteria for good forecasting. This led to experimental designs that were not orthogonal.

Box's designs were built from the multidimensional regular and semi-regular figures. The designs can be sequentially constructed, often beginning with the 2^k and 2^{k-p} designs. These designs are economical in the number of runs, often form unique mixed level fractional factorials, can be orthogonally blocked; and perhaps most appealing to the experimenter, the designs provide configurations of points comprehensible to anyone with a geometric perspective.

For any experimental design and model it is always possible to rewrite the model so that the estimates of the coefficients, or contrasts, in the modified model will be orthogonal. The original model must be reassembled and used as a forecast function. The variance of the forecast over the experimental region is important. Beyond orthogonality, rotatability and robustness to the biases due to unestimated higher order terms are the essential keys to good design.

The 2^k and 2^{k-p} Factorial Design:

A reasonable experimenter will wish to collect data at more than the N = k+1 experimental points required to fit the first order model so as to provide measures of possible second order terms (i.e. to provide measures of the lack of fit of the first order model). Popular designs for this purpose are the 2^k factorials, and for k more than 4, the fractional factorial designs. The 2-level fractional factorial designs are particularly useful as 'factor screening' designs. One objective of the experimenter is to identify those few factors from a

large group that have the greatest influences upon the response over the ranges studied.

The 2^{k-p}_{III} resolution III designs are strictly first order designs since 2-factor interactions, should they exist, directly bias the first order estimates. Resolution IV designs, the 2^{k-p}_{IV}, provide first order estimates clear of second order biases. Resolution V designs, the 2^{k-p}_{V}, provide seperate orthogonal estimates of all first order effects along with all k(k+1)/2 two-factor interaction effects.

The 2^{k-p} fractional factorial designs cannot provide estimates of the quadratic coefficients β_{ii}. Fortunately, the inability to estimate these coefficients does not bias (alias) either the estimated first order or interaction effects. The β_{ii} terms, if they exist, bias only the constant term. To provide some indication of possible quadratic influences, a center point is often added to the 2^{k-p} design. If the average response at the center is significantly different from the average of the remaining observations, then the quadratic influences must be considered. If on the contrary, any of the x_i's are qualitative, then no quadratic influence is possible.

The 2^{k-p} fractional designs are easy to generate. Their different biases (alias structures with corrected sign) are easily obtained from the design generators. The method is simple, straight forward and general in approach [Box, Hunter and Hunter,1978]. The folding over(i.e. combining the design with a repetition of the design having the

plus and minus levels of the factors reversed) resolution III designs produces designs of resolution IV. Each added fraction can be chosen with an eye to elucidating the effects of the important estimates and for reducing biases. Individual biases can be separated with very few runs. By proceeding sequentially, a huge array of possible fractional factorials is obtainable. It is wise not to plan too large an experimental design to begin with, but to proceed as the data enlightens.

The 2^{k-p} fractional factorial designs are also easy to analyze. Yates algorithm can be employed to estimate the many factorial effects and to construct the associated ANOVA table. The algorithm may also be used in inverse fashion to provide estimates of the response once leading factors have been identified. Normal plots and half-normal plots can be used to help identify leading effects and to study residuals.

Second Order Models and Designs:

When the experimenter thinks in a one-factor-at-a-time fashion, curvature becomes the departure from the simple first order, straight line , model. Here, the only second order coefficient is the quadratic coefficient β_{11}, in the model $\eta = \beta_0 + \beta_1 x_1 + \beta_{11} x_{11}^2$. But when the experimenter contemplates the response as a possible second order function in k factors his model contains, in addition to the k quadratic(curvature) coefficients β_{ii}, an additional k(k+1)/2

crossproduct(two factor interaction) coefficients β_{ij}. For example, when k=5, there are 10 cross product but only 5 quadratic coefficients.

Choosing a good second order design:

Before discussing experimental designs capable of estimating all (k+1)(k+2)/2 coefficients in a second order model, let us consider the problem of empirically modeling a k factor response function. The best way is to begin with a 2^k factorial or 2^{k-p} fractional factorial and attempt initially to identify those factors with important first order effects(or coefficients β_i).

A good screening design provides sufficient additional experimental runs so that the adequacy of the first order model can be appraised. Should estimates of either quadratic or the 2-factor interactions appear important, the experimenter should then try to reduce these influences by making transformations in the data. Very often, transformation of the observations using reciprocals or logarithms will eliminate the need for the interaction or the quadratic terms in the model. The objective should be to make the approximating model as simple as possible. If first order simplicity is not possible, the two level factorial already completed can be used as a building block for a second order design, possibly an orthogonally blocked central composite rotatable design.

This approach is sequential and may not be practical in some instances. In that case usually a 2^{k-p} resolution IV design with a repeated center point, is used. Pareto's principle (the selection of the few largest influences) is then used to simplify ones understanding of which factors most affect the response in both a first and second order sense.

V. FRACTIONAL FACTORIALS ANALYSES

One of the problems that frequently is a stumbling block in the effort to improve quality in industry is the problem of trying to identify and isolate the key variables that cause appreciable changes in the quality characteristic of the product or of the process. [Meyer, 1986]

One way of trying to find a feasible solution to this problem, is to resort to the use of fractional factorial statistical design methods. At the preliminary stages of an investigation, a 2-level fractional factorial is used as a screening design.Let us consider a 2-level design. [Box and Hunter, 1961] It is assumed that the design matrix, $\underline{X}$ is an n x n orthogonal matrix of negative or positive ones such that $\underline{X}'.\underline{X} = \underline{X}.\underline{X}' = n.\underline{I}$. The first column of the orthogonal matrix is Xo, which is a column of ones and the rest of the columns are arrays of -1 or +1 representing levels of the experimental variables; -1 represents the lower nominal level and +1 denotes the higher or alternate level of a factor.

At the preliminary stages of the investigation, a first order model in main effects alone is often adequate. If V is the number of variables, the model can be written as $\underline{y} = \sum_{j=0}^{V} \underline{x}_j.\underline{\beta}_j + \underline{\epsilon}$; with the elements of $\underline{\epsilon}$ assumed to be independent and normally distributed with mean 0 and a constant variance. The main effect of variable j is defined to be twice the regression coefficient $\underline{\beta}_j$. If this model is true, the parameters including the error variance can be estimated efficiently (if (n-1)-V is large enough to provide the desired

degrees of freedom for estimating variance). This form of model would fit the situation when the response surface is roughly planar over the experimental region examined. If however, the response surface is closer to a second order function, then

$$\underset{\sim}{y} = \underset{\sim}{x}_0\underset{\sim}{\beta}_0 + \sum_{j=1} \underset{\sim}{x}_j\underset{\sim}{\beta}_j + \Sigma\ (\underset{\sim}{x}_i\underset{\sim}{x}_j)\ \beta_{ij} + \underset{\sim}{\epsilon}\ ,$$

is a better model to postulate. For this model, the estimate of the mean $\underset{\sim}{\beta}_0$ would be confounded with the pure quadratic coefficients, β_{jj}. Also, the linear estimates of $\underset{\sim}{\beta}_j$'s would be confounded with the interaction terms β_{ij}. The estimate of the error- variance, supplied by [(n-1)-V] unassigned columns might be biased by the real interaction effects.

To circumvent this problem, a 2nd. order design can be employed, one that allows estimation of all the parameters of the model. This will however greatly reduce the number of factors that can be analysed in a given number of runs. Another approach is to use the 'sparsity effect'. If it is possible to identify the key factors that cause appreciable changes in the product characteristics, then even if the true response were more closely approximated by the 2nd. order model, the effects of many of the parameters would then be negligible compared to those of the parameters that were associated with the isolated variables and the noise effects. This approach, while combining the good points of relative inexpense and greater information, also goes further in that it does not produce un-ambiguous results.

In the analysis of fractional factorial experiments, one has to identify and estimate the active effects and the error variance.

One of the techniques employed to identify the active contrasts is to use the ANOVA in order to judge the reality of the contrasts. Here, this method relies on comparing the contrasts with an independent measure of the error variance. If an estimate of the error variance is available from past information, the construction of the ANOVA table is relatively simple. If the error variance is not estimable initially, then it is usual to isolate apriori certain contrasts that have only higher order interactions whose magnitudes could be wholly attributed to random error. These contrasts are then used to estimate the error variance. This method or technique restricts the degree of fractionation usable in the design, since many of the columns must be reserved to estimate effects that are known to be inert. If however, little is known about the effects, [i.e. whether they are inert or not], the contrasts might be difficult to identify.

A normal plot is used to judge the significance of the orthogonal contrasts from the factorial experiment. Here, the (n-1) signed ordered contrasts are plotted against $\phi^{-1}[(i-.5)/(n-1)]$, where ϕ is the standard normal distribution function.[Daniel, 1976] Under the hypothesis(Ho) of no active contrasts, the points should fall in an approximate straight line passing through the origin. Too large contrasts that cannot be explained by noise would appear as extreme points falling off the line. The advantage of the normal plot is that it does not require replicated runs or apriori identification of inert contrasts. Additionally, it allows for the selection of

significant effects. The normal plot can be used to test inadequacies in the model.

The next task is to associate factorial effects with active contrasts in the presence of confounding.

- Main effects are more likely to occur than 2-factor interactions, which in turn are more likely to occur than 3-factor interactions. i.e. if a large contrast is associated with more than one effect, the effect of the lowest order is usually considered the most likely cause. In general, we ignore 3-factor and higher order interactions.
- Variables which have large main effects have a greater chance of having significant interactions. eg. when a large contrast is associated with several 2-factor interactions, the interaction involving variables with large main effects are more likely to be the cause.

Identification of active contrasts

To model the assumption that a majority of the **column contrasts** are expected to be inert, we assume that there is a prior probability α that each column is active, where α is less than 0.5. Let a(c) be the event that a particular combination of the **c contrasts** out of the (n-1) are active, the others being inert. The prior probability of the event a(c) is

$$P[a(c)] = \alpha^{c}[1-\alpha]^{(n-1-c)}$$

After observing the data(y), the posterior probability of the event a(c) is

$$P[a(c)/y] = [P(y/a(c)).P(a(c))]/[\sum_{I} P(y/a(i)).P(a(i))].$$

The denominator is the sum over all possible combinations of active and inert columns and P(y/a(c)) is the predictive density of observations(y) given a(c). The marginal posterior probability that column(i) is active =

$$p_i = P[\text{column i active}/\ y] = \sum_{(c):i\ \text{active}} P(a(c)/y).$$

The inference about which columns might be active , can then be made from the knowledge of $[p_i]$. [Meyer, 1986]

Identification of active factors that are responsible for large contrasts

It is assumed that the **factors** will be active with probability α. {a(f)} is the event that a particular combination of the **factors (including interactions)** is active. the posterior probability of each factor being active is then given by

$$p_i^* = \sum_{(f):i\ \text{active}} P(a(f)/y).$$

The results of unreplicated fractional factorial experiments are found to be sensitive to incorrect observations. Daniel[1976] estimated that the relative frequency of bad values in factorial experiments was between 0.01 and 0.1, depending on the complexity of the experimental situation and on the experience of the experimenter. He also felt that the presence of higher order interactions was more often due to erroneous observations than to the curvature of the response surfaces. Due to the saturated nature of unreplicated factorial experiments, bad observations can be concealed by mis-identifying them with some combination of active effects.

Full normal plotting of observational contrasts can be used for detecting bad values in unreplicated experiments, additionally it can be used to identify active contrasts. If a particular observation is biased positively, those contrasts in which the observation enters positively are shifted rightwards, and those contrasts in which the observation enters negatively are shifted leftwards creating a gap among inert contrasts indicating that a bad observation is present.

Presently, the amount of computing time needed to analyse for bad values in full generality, is not practical. Based on reasonable assumptions about the maximum possible number of bad values and active contrasts, various computational shortcuts may be used. It is hoped that future advances in computing technology will reduce these limitations.

Unreplicated factorial designs have been invaluable in industrial experimentation, despite the fact that they do not allow for the estimation of error variance obtained from replications. If assumptions are made about the sparsity of real effects and these are incorporated into the linear model used for the experiments, the inference about the active and the inert contrasts is straight forward and the dependence of the inference on the prior assumption is easily assessed. Bayes' factorization allows the posterior probability to be obtained by numerical integration at a considerable reduction in computing requirements. The model can be made to allow for bad observations which cause an inflated variance with a prior probability of α_2. Given this model, the posterior probability that a

contrast/factor is active can be found by taking into account the possibility of having bad observations as well as the probability that a particular observation has an inflated variance.

VI. ORTHOGONAL ARRAYS SOLUTION AND A NON-LINEAR METHOD SOLUTION TO THE WHEATSTONE BRIDGE PARAMETER DESIGN PROBLEM

Parameter Design is a technique aimed at reducing variation by **reducing** the sensitivity of an engineering design or product to sources of variation rather than by controlling these sources.

The reproducibility of a quality characteristic of a manufactured product inevitably depends on the amount of variation in the components that are in the product.The process of choosing some values for the components' characteristics, that minimize the effects that are transmitted to the product's quality, is called **PARAMETER DESIGN.** Illustrated below, is an example of a parameter design problem. (Taguchi and Wu,1980)

Consider a Wheatstone Bridge(circuit diagram shown in fig.1), used in determining an unknown resistance, R_0. This circuit is used in some product to be manufactured and the design specifications of its components are such that optimal precision must be achieved in measuring R_0. Variable resistance B, can be adjusted so that the galvanometer is balanced, indicating that there is no current flowing through the resistance, A. The resistance y, that estimates Ro is calculated as y= B.D/C , when no current in the central element exists. However, in general

$$y = f[A,B,C,D,E,F,X] \qquad (1a)$$

$$= B.D/C - \{X/(C^2.E)\}[A(C+D)+D(B+C)][B(C+D)+F(B+C)]. \qquad (1b)$$

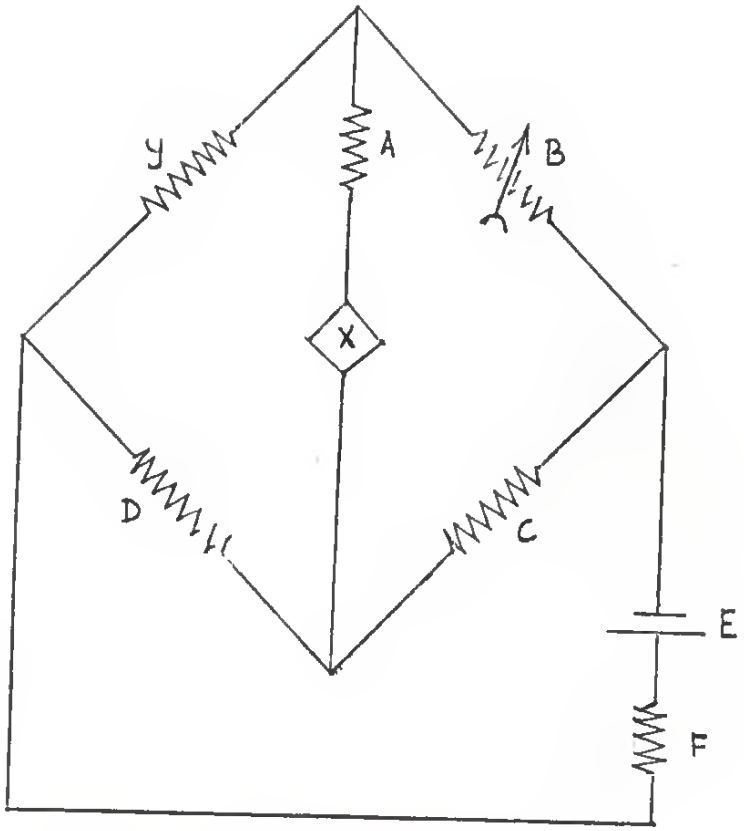

Figure 1. The Wheatstone Bridge Circuit.

For a particular set of components, each of the component can deviate slightly from its target or nominal value, thereby causing an error in the calculated value of y. It must be noted that the values of A,B,C,D,E,F are at a choice; thus the error caused due to various choices would vary accordingly.

The parameter design problem is to find the nominal values of the design variables such that the error shown in the measured value of y is small. In this example, the design factors are those from which we

can choose the nominal values (A,B,C,D,E,F), and the noise factors refer to those factors that cause an error in y. Typically the range of possible nominal values of each design factor is known apriori and is usually quite wide; it is denoted by 'R'.The coefficient of variation is also supposed to be known for each factor.This measures the variation to be expected about a nominal value, from one observation to the next. Compared to the allowable ranges , the coefficient of variations are mostly small.

The objective function that has to be maximized is

$$SNT = 10 \log[\bar{y}^2/s_y^2 - 1/36] \qquad (2)$$

For this problem, maximizing SNT is equivalent to minimizing $SN = \bar{y}^2/s_y^2$. A solution proposed by Taguchi was based on a double application of an orthogonal array design. An inner array was chosen for the design factors(A,C,D,E,F) and combinations of 3 levels for each of the 5 design factors were used in the orthogonal array, L36.(See table 2) The design was made to span the space,R, of the whole design region of interest. At each of these inner array points, an outer array of L36 was used where seven noise factors were allowed to vary over small ranges. Thus,the full layout consists of 36 x 36 points= 1296 points. (See figure 2).

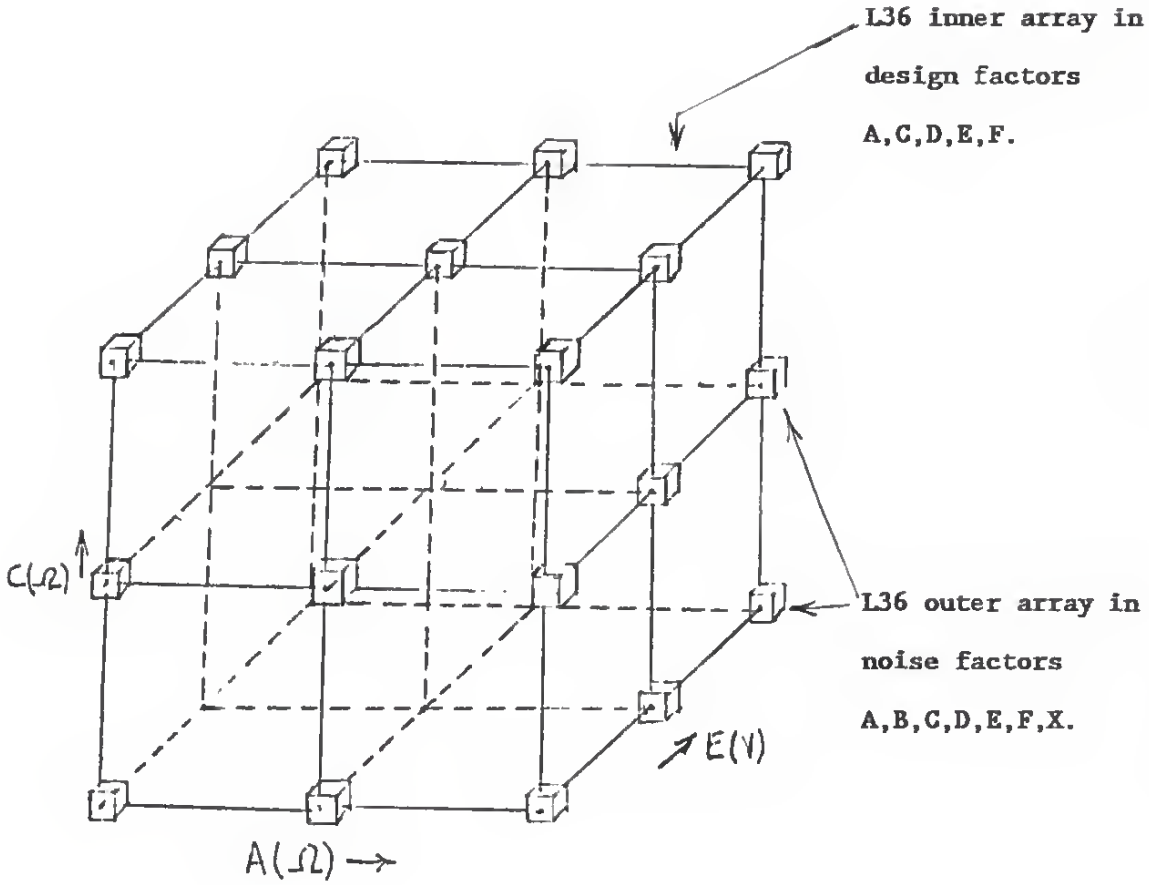

Figure 2. Conceptual illustration of how outer arrays are arranged at each inner array point.

The SNT values from equation 2, were calculated for each of the 36 inner array points and the results were subjected to An analysis of variance. The Marginal averages were calculated for the 3 levels of each design factor and plotted. From inspection of these marginal

averages, Taguchi was able to determine the levels of the factors(A1C3D2E3F1) that maximized the objective function, y =f(A,B,C,D,E,F,X). However, direct calculations show that this was not the factor combination that corresponded to the maximum SNT. The factor combination A1C2D2E3F1 gave a higher value of SNT. The reason for this discrepancy was that, the plots for C and D were curvilinear. For the linear plots of A,E,F the optimal values would be A1,E3,F1. However, optimal values for C and D are not immediately obvious. Due to the dominant effect of the quantity, D/C, in the equation for y, the maximum is found to be considerably different from that found by the orthogonal array method.

A more general form to this problem: [Box and Fung, 1986]

Let H= - log(SN). The problem is essentially to minimize H, which, in essence, is a known function of the levels of the design factors. In other words, one has to minimize a known function(H) within a known region. Let x_i denote the i^{th} factor and consider the following:

1. A d-dimensional design vector x_d, whose elements are chosen arbitrarily from a region R.

2. A k-dimensional vector x_e, some or all of whose members may be the same as in x_d and whose covariance matrix is Σ_e. Let the elements of x_e vary independently such that Σ_e is diagonal with the (i,i) element= σ_i^2.

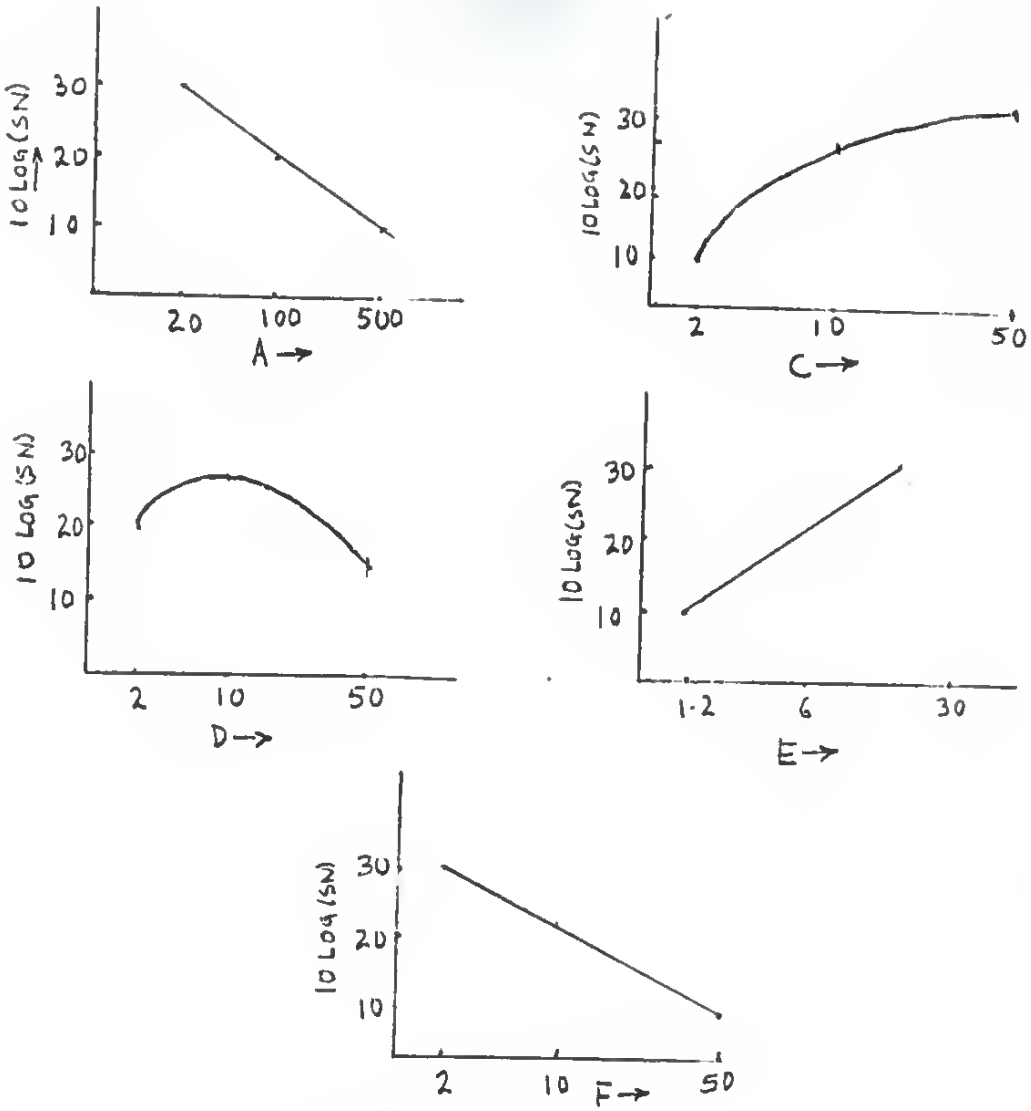

Figure 3. Plot of the Marginal Averages.

3. Define a function, H, which, given Σ_e, can be computed and which measures the relative variation in y, such that the relation, $y = f(x_e)$ is known.

The point is to minimize a known function, H within a known region(R). What are the different methods of doing this and what are their relative merits?

The efficacy of the Taguchi technique in comparison to a non-linear programming for the Wheatstone Bridge example:

The reciprocal of the signal to noise ratio(SN) is the square of the coefficient of variation of y and for some relatively small errors encountered in this example, the coefficient of variation is to a very good approximation equal to the standard deviation of ln y. Therefore, this is essentially a problem of minimizing V_x, the variance of $\ln(y_x)$. Where ,

$y_x = f(\underline{x}) = f(x_1, x_2, \ldots, x_7)$, the x_i's denote the noise factors as A,B,C...etc. and let (4)

$$Y_x = F(\underline{x}) = \ln f(\underline{x}) \quad (5)$$

Given the small error variations in the x_i's, in this example, the variance of Y_x is closely approximated by the first order error transmission formula,

$$V_x = d_1^2(\underline{x}).\sigma_1^2 + d_2^2(\underline{x}).\sigma_2^2 + \ldots + d_7^2(\underline{x}).\sigma_7^2 \quad (6)$$

Where, $d_i(\underline{x}) = (\delta F/\delta x_i) \Big|_{\underline{x}}$ $\quad i = 1,2..,7.$

In other words, the problem is to minimize a known function of V_x or equivalently, $H'_x = 10.\log V_x$ within the known design region R' defined by: $-1 < x_i < +1$ $\quad i=1,3,4,5,6$. The d's could be determined by direct differentiation, but in practice they are calculated using nonlinear least square techniques.

$d_i(\underline{x})$ is approximately $= [F(x_1, x_2, .., x_i+\delta, .., x_7) - F(\underline{x})]/\delta_i$ (7)

where, the level of the i^{th} factor is incremented by δ_i and all other factors are held fixed. δ_i is such that it is proportional to σ_i, the constant of proportionality being some α. If $y = F(\underline{x})$ has different values $Y_0, Y_1, \ldots, Y_k$ at the different design points then equation 6 takes the form:

$$V_x \text{ is approximately} = (1/\alpha^2)\{(Y_1-Y_0)^2+(Y_2-Y_0)^2+\ldots+(Y_k-Y_0)^2\} \quad (8)$$

Thus, the equivalent objective function is $H'_x = 10.\log V_x$ (9)

When the two methods were compared, holding α fixed at 1/1000 to compute H'_x for all the 36 points in the L36 inner array, it was found that both methods closely tallied or agreed with each other. See table 2 for the comparison.

In general, the degree of difficulty associated with minimizing H(x), within a region R depends a. on the complexity and the dimension of R and b. on the complexity of H(x) itself.

Design region R is defined by a series of simple ranges for each design factor. When a direction of advance affects a constraint defining R, the possibility of changing the system so that R may be extended, is considered.

As far as the nature of H(x) is concerned, much depends on the extent to which the smoothness properties of the function can be relied upon. For parameter design, the function

$$V_x = d_1^2(x).\sigma_1^2+d_2^2(x).\sigma_2^2+\ldots+d_7^2(x)$$

where $d_i(x)=(\partial F/\partial x_i)]_x$; i=1,2,..7

has some useful properties. It is positive except at x_0 where $d_1(x_0)= d_2(x_0)=...=d_k(x_0)=0$. i.e. if there is a point, x_0, internal to the region it must be a minimum for V_x(not nessecarily unique.) and is a stationary point of the response function F(x). If for a region 0, F(x) represents a 2nd degree polynomial in x, then V_x is also a 2nd degree polynomial over the region 0 and in addition is convex. For the Wheatstone Bridge case, the three factors behaved roughly linearly and the other two were quadratic. In general,however, it may be more difficult to find the values of x to minimize V_x. Should a survey of V_x over the whole design region be made or should it be confined to the local properties of the function? If the former were to be done, it might prove to be expensive to achieve a grid of points of sufficient density. For complex functions, if the latter were to be done, then the local properties could mislead.

Taguchi's solution uses 'Global Exploration' on a 3-level orthogonal grid. No provision is made for the interaction between the variables and the marginal graphs are used in finding a maximum H(x). This works especially well if H_x is like a sum of independent quadratic components or a general 2nd degree expression without any interaction terms. However, the need for second order terms of one kind(quadratic) in an approximating function implies the likely need for 2nd order terms of the other kind(2 factor interactions). Thus, when dealing with continuous variables and when we are interested in

the maxima, it is not sufficient to consider 2nd order terms of one kind and not those of the other kind. If 2nd degree approximations are considered, a design that allows for an estimation of 2 factor interactions as well as quadratic terms, needs to be employed.

Random Search:

In most cases, the precise dimensions of the design region and hence the precise values of σ_i's are not known exactly. Thus, there is hardly any meaning in trying to obtain a precise optimum. In this case, a choice of design variables that is reasonably good might be the best that can be done. Hence, the determination of H_x at n sample points in R and the selection of a vector(X) that yields the best H_x, may be adequate. Sampling may be done randomly within R or may be systematic on a predecided grid such as an orthogonal array.
Obviously, this method does not yield the best results.
Many optimizing strategies are based on the local properties of the function. One such method initially employs the method of steepest ascent, switching to the fitting of a 2nd order approximating function when the 2nd order terms become important (or when the curvilinear efects predominate.). Simplex technique can also be used for optimization.

These methods along with others seem less arduous and more computer friendly and can be adapted to a wider range of problems in comparison to orthogonal arrays.

Taguchi, in defense of his orthogonal array method, has criticized the efficacy of the more commonly used non-linear programming methods. The points in favor of the Orthogonal Array method are:

i. No derivatives need to be computed.

ii. The Hessian need not be found.

iii. The algorithm is insensitive to the starting conditions.

iv. Large number of variables can be handled.

v. Combinations of discrete and continuous variables can be handled easily.

Table 2.Orthogonal Array L36
The last column shows the values of 10.log(SNT) obtained by Taguchi at the 36 inner array points.(Box and Fung, 1986)

Run no.	Column no. 1	2	3	4	5	6	7	8	9	10	11	12	13	Taguchi's criterion	-H' x
1	-	-	-	-	-	-	-	-	-	-	-	-	-	32.2	32.27
2	0	0	0	0	0	0	0	0	0	0	0	0	-	26.7	26.68
3	+	+	+	+	+	+	+	+	+	+	+	+	-	15.9	15.90
4	-	-	-	-	0	0	0	0	-	-	-	-	-	36.4	36.43
5	0	0	0	0	+	+	+	+	-	-	-	-	-	28.6	28.59
6	+	+	+	+	-	-	-	-	0	0	0	0	-	7.2	7.23
7	-	-	0	+	-	0	+	+	-	0	0	+	-	16.5	16.48
8	0	0	+	-	0	+	-	-	0	+	+	-	-	13.0	13.02
9	+	+	-	0	+	-	0	0	+	-	-	0	-	28.0	28.05
10	-	-	+	0	-	+	0	+	0	-	+	0	-	15.0	15.07
11	0	0	-	+	0	-	+	-	+	0	-	+	-	16.4	16.47
12	+	+	0	-	+	0	-	0	-	+	0	-	-	25.5	25.55
13	-	0	+	-	+	0	-	+	+	0	-	0	0	43.8	43.75
14	0	+	-	0	-	+	0	-	-	+	0	+	0	-8.3	-8.28
15	+	-	0	+	0	-	+	0	0	-	+	-	0	14.6	14.60
16	-	0	+	0	-	-	+	0	+	+	0	-	0	29.0	28.98
17	0	+	-	+	0	0	-	+	-	-	+	0	0	6.9	6.93
18	+	-	0	-	+	+	0	-	0	0	-	+	0	14.7	14.70
19	-	0	-	+	+	+	-	0	0	-	0	+	0	21.5	21.48
20	0	+	0	-	-	-	0	+	+	0	+	-	0	17.4	17.39
21	+	-	+	0	0	0	+	-	-	+	-	0	0	14.0	13.98
22	-	0	0	+	+	-	0	-	-	+	+	0	0	46.5	46.39
23	0	+	+	-	-	0	+	0	0	-	-	+	0	5.5	5.53
24	+	-	-	0	0	+	-	+	+	0	0	-	0	-8.2	-8.15
25	-	+	0	-	0	+	+	-	+	-	0	0	+	27.3	27.35
26	0	-	+	0	+	-	-	0	-	0	+	+	+	43.4	43.50
27	+	0	-	+	-	0	0	+	0	+	-	-	+	-20.9	-20.85
28	-	+	0	0	0	-	-	+	0	+	-	+	+	44.1	44.09
29	0	-	+	+	+	0	0	-	+	-	0	-	+	39.3	39.33
30	+	0	-	-	-	+	+	0	-	0	+	0	+	-17.0	-17.00
31	-	+	+	+	0	+	0	0	-	0	-	-	+	23.0	23.07
32	0	-	-	-	+	-	+	+	0	+	0	0	+	44.2	44.09
33	+	0	0	0	-	0	-	-	+	-	+	+	+	-0.9	-0.84
34	-	+	-	0	+	0	+	-	0	0	+	-	+	43.4	43.50
35	0	-	0	+	-	+	-	0	+	+	-	0	+	-7.7	-7.64
36	+	0	+	-	0	-	0	+	-	-	0	+	+	8.0	7.97

(Note: Comparison of $-H'_x = -10 \log V_x$ obtained by divided first differences of Y= ln y, against Taguchi's criterion 10 log(SNT) obtained from outer arrays.)

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STATISTICAL TECHNIQUES FOR QUALITY IMPROVEMENT

BY

SRINIVAS B. GARUDACHAR

B.E.(MECH), OSMANIA UNIVERSITY, 1985

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ABSTRACT

Off-line quality control methods activities conducted at the product and process design stages to improve product manufacturability and reliability, and to reduce product development and lifetime costs. Parameter design is an off-line quality control method. At the product design stage, the goal of parameter design is to identify settings of product design characteristics that make the product's performance less sensitive to the effects of environmental variables, deterioration, and manufacturing variations. Because parameter design reduces performance variation by reducing the influence of the sources of variation rather than by controlling them it is a very cost-effective technique for improving product quality.

This paper introduces the concepts of off-line quality control and parameter design and discusses the Taguchi Method for conducting parameter design experiments. Also, included are some criticisms of the Taguchi Method.